Promoting Scholarship that Matters: The Uselessness of Useful Research and the Usefulness of Useless Research

Our ambition in this essay is to challenge received wisdoms about the importance of "useful" management scholarship. Suggesting that usefulness and uselessness are contingent on issues of temporality and power, we advocate caution in assigning terms such as useful and relevant – they are inherently problematic, we argue, and should be viewed more as ideology than as empirical statements. We conclude by a call for reflexivity about what it is we are doing when we do "useful" research, along with a greater concern for the values for which business schools stand.

Authors: Mark Learmonth, Andy Lockett and Kevin Dowd.

Introduction

In this essay we seek to provoke debate by questioning an increasingly received orthodoxy in business schools: that management education and research should be 'useful' (Baldridge, Floyd and Markóczy, 2004; Starkey and Madan, 2001). Not that we are defending uselessness – we want to promote 'scholarship that matters' (Őzbilgin, 2010). What we aim to do, however, is to question *orthodox* ideas about the uses and application of business schools' outputs; orthodox ideas, we believe, that are becoming so taken-for-granted that their influence is increasingly dysfunctional. Such taken-for-granted ideas, which, in this essay we trace back primarily to a

complex set of inter-relations of power, risk dangerously constraining the range of activities deemed legitimate within business schools (Ford, Harding and Learmonth, 2010). Indeed, a wider manifestation of these assumptions can be seen in the British government's current policy of funding higher education teaching only in science and technology – subjects they deem to be 'useful'.

Provoking debate of this kind is becoming increasingly important for us, personally. Today, as business school academics, there are growing pressures on us to produce work deemed by others to be 'useful', 'impact-ful', 'relevant' and so on. These pressures are increasingly upon us, not least because of the changing ways in which our work is assessed and

valued in the UK's Research Excellence Framework (REF) proposals (Lipsett, 2011) as well as in other, broader cultural changes that influence how academic work is measured and appreciated within wider society. We feel such changes are already beginning to alter the nature of the research we would consider doing; sometimes, they are even making us wonder whether the more our work is judged 'useful' in the terms of measures used in the REF, the less our work might actually matter to us. It is for partly personal reasons, then, that the examples we have chosen to illustrate our points later in the essay (the supposed usefulness of finance research and the supposed uselessness of deconstruction) represent topics of personal interest for two of us. The usefulness (or otherwise) of these modes of research are matters in which we have directly personal stakes.

We see this essay, therefore, in part, as a personal and reflexive exploration (Alvesson and Sköldberg, 2000; Cunliffe, 2003; 2004) of the paradoxes surrounding usefulness and uselessness. But it is also, more broadly, an exploration of what we are doing when we do research, why we might be doing it, and how our work fits in to the power relations within academic life – power relations to which we are subjected and, to which we no doubt contribute. Thus, we aim to add to the ongoing debate about the appropriate nature of business school scholarship (Currie, Knights and Starkey, 2010; Ferlie, McGiven and De Moraes, 2010); and we do so principally by arguing that terms such as relevance/usefulness are *in themselves* inherently problematic. Indeed, we seek to take the debate beyond the issue of whether or not scholarship should be useful, to questioning what lies behind the various constructions of useful/useless that are deployed in the debates. The intent, then, is to become better informed about how the manner in which we frame 'usefulness' affects the ways in which we might endeavour to attend to it.

In developing these arguments we focus on two key arguments. First, we suggest that ideas about usefulness are contingent upon time (Antonacopoulou and Tsoukas, 2002; Augier and March, 2007). What counts as useful is influenced by the changeable fads of management fashions and ideologies (Abrahamson, 1991; Barley and Kunda, 1992; Lowrie and Willmott, 2006) as well as on cultural and technological shifts in wider society. In other words, even (what is conventionally regarded as) uselessness may sometimes turn out, in time, to have its uses. A famous example from another discipline was the work of the great Cambridge mathematician, G. H. Hardy, who towards the end of his life lamented on the uselessness of his life's work on pure mathematics (Hardy, 1940), and who did not live to see it become a key element in the development of modern computing. The converse of this phenomenon is equally important to us – we show how things that are deemed useful in the short run can turn out, over time, to be deemed useless (or worse).

Second, we argue that usefulness is also contingent upon relations of power. Useful management scholarship is commonly that which those with the power to make judgments about it (e.g. business executives or government funding bodies) believe is valuable to their interests or constituencies; activities that do not serve these purposes are then dismissed as useless. In other words, we are suggesting that usefulness has an ideological dimension in the Marxian sense; which is to say that the dominant version of "usefulness" currently in vogue within business schools has been socially and historically constructed to serve the interests of elites, but it has widely come to be seen as necessary, natural, self-evident and unquestionable (Alvesson and Deetz, 1996). So, for example, as a recent editorial in the *Academy of Management Journal* (Bamberger and Pratt, 2010) points out, it is very hard to publish work about non-managers in most management journals, especially those non-managers who are in marginal groups within organizations. In part, according to Bamberger and Pratt (2010, p.666), this is because 'such research can be viewed as "too weird" or too far outside of what people consider appropriate for management research'. But the 'people' doing the considering in this context, are presumably those who have the power to make their judgments count.

Our analyses of the contingent nature of usefulness then, leads us not so much to disagree with the conclusions of Van de Ven and Johnson (2006, p. 803), for whom the work of business schools increasingly 'needs to achieve the dual objectives of applied use and advancing fundamental understanding'; rather our analysis leads us to stress that what constitutes 'applied use' is much more complex, paradoxical and unstable than is often assumed (Aleroff and Knights, 2009; Knights, 2008; Rasche and Behnam, 2009). One reason, perhaps, for the ideal continuing to prove so elusive.

To show how our interpretation of usefulness might apply to contemporary business schools, we then consider two contrasting approaches to management scholarship. One that has widely been seen (until recently) to exemplify the height of usefulness: modern finance; the other, often vilified as useless: deconstruction. We conclude with the suggestion that a key issue in any debate about the usefulness of the work of business schools is the very thing which often gets occluded in discussions of themes such as engagement, relevance and usefulness: the values and ideologies for which we wish our institutions to stand (Antonacopoulou, 2010).

The Useful Debate

One aspect of usefulness – the rigour-relevance debate – is well established in management research; the traditional argument being that there is a trade-off between rigour and relevance. Indeed, debates about the purpose of business school scholarship have often tended to polarize around ideas about 'the soldiers of organizational performance and the priests of research purity' (March and Sutton, 1997, p. 703;

and see Peng and Dess, 2010, for a review of the competing camps). The former have sought to define the relevance/usefulness of business school scholarship with reference to its effects on improving organizational performance. For them, scholarship should be 'concerned with knowledge as it works in practice in the context of application' (Starkey and Madan, 2001, p. s5; see also Das, 2003). In contrast, the priests of research purity have argued that inside the proverbial "ivory tower," scholars are, by definition, not supposed to be relevant or useful (Kieser and Leiner, 2009; March and Reed, 2000; Peng and Dess, 2010).

More recently a new hybrid position is emerging, wherein the liberal virtues of the traditional university are linked to (market) relevance (Lowrie and Willmott, 2006). Thus, today, many management scholars are arguing that rigour and relevance are not mutually exclusive (Bartunek and Rynes, 2010; Gulati, 2007; Rynes, 2007; Van de Ven, 2007). These kinds of arguments suggest that research can be rigorous *and* relevant, and that 'by probing more deeply into the problems and other issues that managers care about, we can naturally align our interests with more practice-relevant research, without sacrificing rigor.' (Gulati, 2007, p.780). Gulati outlines five different practices that will enable researchers to bridge the divide between rigour and relevance. One such practice, for example, states that managerial sensibilities should shape research questions. Here Gulati draws on Lawrence's (1992) notion of "problem-oriented"

research, or work that focuses on real-world managerial challenges: '[o]ur subjects [i.e. managers] can tell us what needs to be studied—where our theories and knowledge are inadequate' (Lawrence, 1992, p.140). A second example from Gulati (2007, p.780) relates to testing theory in the classroom, where he argues that 'most business school students and business executives we teach in our burgeoning executive education programs are past, current, and/or future managers, so there is no better group on which to test the relevance and potential value of theoretical concepts.'

In other words, for Gulati and others in similar traditions, by bringing academics and managers together to foster dialogue and shape the focus of research activity, academics will produce more "relevant" research. For these scholars, the relevance gap is bridged by moving from a so-called Mode 1 form of scholarly endeavour, where the knowledge production is academic and discipline led, to Mode 2, where knowledge production is problem focused and interdisciplinary (see: Gibbons et al., 1994). Under Mode 2, knowledge is co-produced through interaction between the user and the academic, and therefore has greater applied use.

For us, however, one of the major problems with this kind of argument is that terms such as "relevance" and "use" are employed unproblematically. But as Weick (2001, p.s71), suggests, the much lamented relevance gap persists because we tend to 'forget that 'the' real world is actually 'a' world that is idiosyncratic, egocentric and

unique to each person complaining about relevance.' Simply stated, people see relevance in different ways. Thus, relevance for Weick, as for us, is inherently contestable and, as such, should not be treated as an unproblematic concept.

Interestingly, and keying into a wider debate surrounding Science Technology and Society (STS), the term co-production (or co-evolution) has been used as shorthand for the proposition that the ways in which we know and represent the world (both nature and society) are inseparable from the ways in which we choose to live in it (Jasanoff, 2004). STS scholars therefore view the descriptions of Mode 1 and Mode 2 research as being overly simplistic, and as presenting a normative and political ideology of how research should be done (see Godin, 1998; Shin, 2002). The STS view of co-production critiques the realist ideology that persistently separates the domains of nature, facts, objectivity, reason and policy from those of culture, values, subjectivity, emotion and politics (Jasanoff, 2004). Drawing on the breadth of social science perspectives, STS scholars who focus on the co-production of knowledge, have highlighted (not exhaustively) the importance of power, authority and subjectivity to our understanding of scientific knowledge production and consumption in society. As such, the STS perspective on co-production brings into sharp relief the tensions that may arise between the producers and consumers of research.

Temporality, Power and the Role of the Business School

Inspired especially by these wider debates in STS, we develop further the idea that terms like relevance and usefulness are inherently problematic. As they can only ever be subjectively defined, their use necessarily raises the question, 'who gets to define usefulness?' A question, of course, that takes us back to power – one of the issues with which we started this essay. Thus, in adding to the STS literature, we argue that attempts to classify something as useful or useless are necessarily contingent both on the time period over which use is being defined – as well as upon relations of power – the dominant ideology through which scholarship comes to be counted as useful (or useless).

From a temporal perspective, research that is pursued purely out of academic curiosity, without a specific applied use in mind may, as Flexner (1939, p. 544) famously commented, nevertheless prove 'unexpectedly [to be] the source from which undreamed-of utility is derived' (Flexner, 1939, p. 544). (For discussions of this ideas as applied to business school activities, see for example: Grey (2001) and Kilduff and Kelemen (2001).) However, even were scholars to become fully aware of the contingent and unstable nature of usefulness from the point of view of time, because of power relations, the short-run attractions of pursuing usefulness would nevertheless remain extremely seductive – indeed, they are becoming hegemonic. Demonstrating "usefulness" (or relevance) is increasingly important to attract

resources (e.g. donors, funders, and students) and is also an important criterion for the review of submissions to many scholarly journals. Thus, as Willmott (2003, p. 137) argues:

> Increasingly, universities are directly or indirectly dependent upon industry to support or sponsor research (and teaching) activity. ... For academics, demonstrating the relevance of universities for meeting 'the needs of industry' improves the prospect of attracting funding from the state as well as private sector sponsors.

In essence, then, academics in business schools are in a double-bind. On the one hand, we must do work that is deemed useful to gain legitimacy from key powerful stakeholders who are influential in allocating resource and rewards. However, if business schools exclusively pursue (so-called) useful work they may end up becoming useless in the long-run. Without (so-called) useless activities there will be nothing to create 'movement' within the field. Below, we illustrate some of the dangers of supposed usefulness as well as the attractions of supposed uselessness by contrasting two contemporary bodies of theory in management studies – finance and deconstruction.

Modern Finance: The epitome of the uselessness of usefulness

It was widely argued – at least until recently – that the most useful domain of research emanating from Business Schools (as defined by the extent of the direct application of research by practitioners) was finance (AACSB, 2008; Currie, Starkey and Knights, 2010; Starkey and Tiratsoo, 2007). This subject originated in the 1950s: its essence was to apply quantitative methods (often colloquially known as rocket science) to financial problems. (For more on this subject, and on its usefulness, see, e.g., Taleb 2008; Triana, 2009; Dowd and Hutchinson, 2010.) At least to certain elites, finance promised the benefits of better valuation, higher financial returns, lower risks and greater financial stability – indeed, several professors of Finance have been honoured with the Nobel Prize for Economics over the years. Yet almost every major development in this area illustrates how supposedly useful financial research has turned out, given time, to be useless.

Perhaps the most apt current example[1] following the recent

---

[1] Amongst other examples of finance research that was initially received as useful (indeed, several of their inventors have been honoured with the Nobel Prize for Economics) are the Capital Asset Pricing Model, the Modigliani-Miller theorem on firm capital structure, and the Black-Scholes theory of option valuation. The first of these was discredited in the early 1990s, and the credibility of the remaining two has taken repeated beatings: Modigliani-Miller was a key factor in the growth of excess leverage (or risk taking), which was a major contributory factor to the recent crisis; for their part, unreliable options valuations

financial collapse is Collateralised Debt Obligations (CDOs), in which pools of bank assets (such as mortgages) would be assembled and then claims on those assets sold off to investors. These claims would be tranched (or ranked in terms of seniority), so that senior tranches only took losses after the junior ones had been wiped out. In theory, a CDO could be composed of rubbish quality assets, and yet the tranching ensured that the senior tranches were very safe – a kind of financial alchemy; at the same time, the creation of CDOs was widely hailed as enabling borrowers with poor credit ratings to obtain mortgages they could not otherwise have obtained, subprime being an obvious example.

However, when CDOs were first mooted the market was held back by the absence of a suitable model to value these securities and assess their risks. The breakthrough came with a landmark paper by David X. Li (2000), which proposed to value CDOs using a model known as a Gaussian copula, which could be calibrated using historical data on defaults. The publication of the Gaussian copula (even though its publication was in an academic journal) was received by finance practitioners as supremely useful. It allowed the CDO market to take off: by 2008, the size of the CDO market in the US had grown to over $10 trillion dollars or just over 70% of US gross domestic product. Dr. Li himself was soon regarded as a potential Nobelist.

have been a recurrent feature in the long catalogue of financial scandals of the last 25 years.

From the perspective of the short run interests of the powerful within the financial sector, the "usefulness" of CDOs appeared to be enormous. Borrowers got better access to finance, whereas investors got access to new types of investment assets and the prospect of higher returns and greater risk diversification. With the passage of time and new events emerging, however, this apparent usefulness turned out somewhat differently. Large numbers of borrowers were unable to repay and lost their homes, whilst investors lost vast amounts on CDO portfolios whose values collapsed in 2007-2009: 'supersafe' CDOs turned out to be supertoxic. It also turned out that the model gave unreliable valuations and risk assessments because the historical data used did not encompass any major housing downturn – and thus the ultra sophisticated Li model was blind to the most important risk involved. The market for these financial 'products' grew to enormous size not just because participants failed to appreciate their dangers. The key driver was financiers' short-term interests in profits: these securities were extremely lucrative for those who designed and sold them, and for the senior bank managements who lived off the profits that the designers and salespeople generated. Also complicit were the ratings agencies, which were driven by the same short-term profit considerations to give dubious securitizations highly inflated ratings: if the market collapsed later on, that was not their concern.

We suggest, therefore, that this aspect of finance provides an exemplar of the unstable and paradoxical nature of "usefulness". Prior to the financial crisis, the usefulness claimed for CDOs was more or less axiomatic – largely because, as we can now see, its "usefulness" to elites rendered the theory on which CDOs were built immune to serious questioning. But it was this supreme "usefulness" that itself led to disaster in the longer run. Had the model been seen as less useful, the prospects of academic critique exposing its weaknesses before it could do too much damage would have been higher, because there would have been fewer powerful voices with an interest in making sure any critique got ignored. It was, paradoxically then, its very "usefulness" that meant what happened instead was that the weaknesses of the model were only exposed by a disastrous market downturn. Today, the seriousness of the financial crisis that CDOs caused has had the effect of realigning power relations to some extent at least – financiers' definitions of the usefulness of such products are no longer quite so hegemonic. However, with the benefit of a longer run perspective, most of us would now prefer such power realignment to have happened rather earlier than it did!

Deconstruction: The usefulness of uselessness?

Deconstruction, associated with the thinking of the French philosopher Jacques Derrida, has yet to be widely received as useful for management. We use it as an illustrative comparison rather than a direct one, in that deconstruction stands in stark contrast to finance research, having met with more than its fair share of *ad hominem* attacks, both in management circles (see Kilduff and Mehra, 1997 for a discussion), as well as in its home discipline of philosophy (Derrida, 1995, p. 419-20). Typically, Derrida's *ad hominem* critics accuse deconstruction of a nihilism that threatens to undermine rationality and ethics. However, as Kilduff argues, deconstruction 'is used, not to abolish truth, science, logic, and philosophy, but to question how these concepts are present in texts and how they are employed to systematically exclude certain categories of thought and communication' (1993, p.15; see also Cooper, 1989; Jones, 2004). Deconstruction is particularly interesting in the context of management scholarship, therefore, because it aims to produce a tension between what a text purports to claim (its intended meaning) and a double or multiple range of meanings that cannot be contained within the text's intended meaning (Critchley, 2005). In other words, deconstruction offers management scholars possibilities for re-reading our discipline's established facts and taken-for-granted assumptions, in order to start to see their paradoxes, blind spots and double-binds (McQuillan, 2000). Indeed, the revealing of paradox, or rather, the revealing of aporia (paradoxes which are logically irresolvable and therefore have to remain excluded and unquestioned for arguments to appear coherent) is a key

contribution in Derrida's work (Derrida, 1993).

Deconstruction also provides another example of the unstable and paradoxical nature of usefulness (or in this case, perhaps, uselessness). This is because one might speculate that some of the vilification deconstruction has received for its uselessness is symptomatic of the deeper anxieties suffered by those people who enjoy the power necessary to get their criticisms of deconstruction taken seriously (cf. Vince, 2010). After all, it is meant to be radically subversive to dominant interests (including, for the purposes of this paper, business executives, government funding agencies and so on). On the other hand, however, as one way of posing the question, 'useful for whom?' its emphasis on the marginalized and unspoken has provided a "useful" inspiration for a number of radicals within management studies (Boje, 1995; Learmonth, 2005; Martin, 1990; Weitzner, 2007) in championing the interest of people who have little or no power in society. Deconstruction, then, might also be seen as a potential antidote to some of the blind spots in finance research, as well as to the self-interests of people like the financiers whose main concern was to make a quick profit.

Furthermore, deconstruction and other radical ideas are useful for their potential to create movement within systems of thought that otherwise are taken for granted. In the sub-field of strategy, for example, Rasche has recently found deconstruction useful for providing an encounter with those paradoxes inherent within strategy-making 'which must be overlooked to make [strategy's] dominant logics seem undeconstructible' (2008, p.116). But, while his work may well be subversive to strategy's established truths, it is this very feature that promises to be useful in the long run, in that its subversiveness holds out the promise of reinvigorating the sub-discipline. A reinvigoration achieved through nurturing radically new forms of theoretical reflection, while fundamentally challenging our currently taken-for-granted assumptions and beliefs.

Nevertheless, there are unlikely to be obvious short-run gains to be had from such challenges – one of the reasons why deconstruction continues to run the risk of getting dismissed as useless. However, without the sort of reinvigoration deconstruction can provide, in the long-run, strategy risks falling into a dogmatic slumber (or into Bettis's [1991, p.315] famous 'straightjacket' perhaps) that will ultimately threaten the field's capacity to be useful – to practitioners or to anyone else. Indeed, today, many now wish that finance research had also received such challenges – before it was too late! And yet of course, ideas such as deconstruction will only survive in business schools if they remain liberal institutions that encourage questioning and debate – with no over-riding concerns for use.

Conclusions: Towards Scholarship that Matters

What have we learnt from this exploration of some of the complexities, paradoxes and instabilities of "usefulness" and "uselessness"? Does it get us any

closer to producing scholarship that matters?

We started the essay by voicing disquiet about the effects on our own scholarship of the REF and other changes to the way academic work is judged. Like Grey (2001, p.s32), our ideal is now to be able to re-imagine usefulness and relevance and to see ourselves 'working with all the complexities of knowledge, free from the demands of relevance – or, more accurately, free from the current restricted, persecuted and persecutory imaginations of what relevance might be.' In pursuit of this ideal, our central insight in the paper – that terms like "usefulness" and "uselessness", "relevance" and "irrelevance" are always relational, never absolute – has proved helpful in suggesting practical ways forward. For instance, one thing we can practically do when we argue about "usefulness" and "uselessness" is always to use them in conjunction with appropriate caveats – to put the terms, as it were, in "scare quotes" – to remind us of the complexities and instabilities, values and ideologies on which assumptions about usefulness and relevance are necessarily based. The alternative – using these terms in ways that imply they are absolutes – is necessarily to impose an ideology; an ideology, which restricts and persecutes what counts as legitimate work.

The kinds of pronouncements we criticise as ideological and restricting are commonly seen in the editorial statements of many academic management journals. As Bartunek and Rynes (2010) show, most of the world's leading management journals require articles to be relevant to practice; similar conditions are also commonly imposed upon grant-holders by research funders (Learmonth and Harding, 2006). As we have drawn heavily on debates about the relevance of scholarship that appear in the *Academy of Management Journal* (*AMJ*) we have taken its guidance to authors as an illustrative example of the problems such statements raise:

> All articles published in the *Academy of Management Journal* must also be **relevant to practice**. The best submissions are those that identify both a compelling management issue and a strong theoretical framework for addressing it. We realize that practical relevance may be rather indirect in some cases; however, authors should be as specific as possible about potential implications (2011; no page number; emphasis in original).

Even though it is the case that authors often include no explicit recommendations for practice (Bartenuk and Rynes, 2010), we still think this kind of representation of relevance is dangerous for two interrelated reasons. The first danger is that the guidance reinforces and legitimates the conventional view of relevance which we have critiqued in this paper – it appears to conceive of relevance as linear, essentialist and technical. There is no hint that what counts as usefulness might be complex, contestable or value-laden as we have argued – even though a

simple addition (perhaps: 'articles … must also be **relevant to practice**' *in some way*) would be all that is required to hint towards such possibilities. Instead, we find the claim that 'practical relevance may be rather indirect in some cases'. In our reading, such a claim works to reinforce the implication that what constitutes relevance is *conceptually* unproblematic. It suggests that relevance is simply what managers would find helpful; acknowledging only that its *attainment* is (sometimes) difficult in the context of advancing complex new theories. Implicit in this statement, then, are unexamined values and ideologies that suggest conservative ideas about the sorts of research we should be doing. Rather than opening up new possibilities for rethinking what might be useful, the guidance seems to reinforce received ideas about what we are doing when we do management research. We are not invited to reflect deeply upon why we might be involved in management scholarship, nor upon how our work fits in to the wider power relations within organizations and society more broadly.

The second danger we see with the guidance is the place *AMJ* occupies within the power relations that underpin scholarly production in business schools. As one of the most prestigious management journals in the world, how *AMJ*'s editors view relevance and usefulness is important to the scholarly community – as scholars, a major part of our influence is the impact of the journals in which our work appears. In parallel with those who control researching

funding bodies then, people like *AMJ*'s editors have power to make their assumptions about relevance count. Indeed, in the face of such power, however much we might wish to reimagine relevance, reimagining it is made significantly more difficult. This is because we may face having to compromise what we really want to say in order to conform to editors' (or funders') ideological commitments about the relevance and usefulness of our work (Learmonth, 2008).

This insight, however, brings us to another implication of the contingent nature of usefulness. That is, highlighting its contingency tends to recast debates about relevance within the business school. It moves us away from (apparently) technical questions: e.g. "how can we be more relevant?" towards questions more explicitly concerned with power and politics: e.g. "whose interests do we want to be useful to?" A stress on the contingent nature of usefulness, in other words, suggests a focus for debating the kind of business schools we want to be part of – in terms of the values for which we stand and the interests that we serve.

Thus, we are not saying scholarship that matters only happens by questioning orthodox ideas; nor that it only gets produced when our work deliberately sets out to be received as useless. What we are suggesting, however, is that we need to move more reflexively towards debating the fundamentals underlying what we are doing when we do research. By promoting reflexivity we want to stimulate academics to think

through their motivations and interests in conducting research.

Of course, such reflexive exercises will always have their blind spots, as Tatli (2011) rightly points out. However, we do not feel it appropriate "to provide explanations for and solutions to" (Tatli, 2011, p.8) the sorts of fundamental questions we pose here. Rather, our arguments suggest the need to nurture wider debate about the values and ideologies within business school scholarship (Currie, Starkey and Knights, 2010; Reedy and Learmonth, 2009) in the context of debates about usefulness and relevance. This is because the increasing reliance on taken-for-granted assumptions about usefulness and relevance tends to do the opposite – to close down debate and obscure the power relationships which underlie what has come to count as useful in the first place. Indeed, should usefulness continue to be seen as an unproblematic good that researchers and educators aim for without examining the assumptions that underlie their thinking, then "usefulness" will, most likely, lead to business schools' (and business's) long-term decline and stagnation.

References

AACSB (The Association to Advance Collegiate Schools of Business) (2008). *Final Report of The AACSEB International Impact Of Research Task Force.* AACSB International, Tampa.

Abrahamson, E. (1991). 'Managerial fads and fashions: the diffusion and rejection of innovations', *Academy of Management Review,* **16** (3) pp. 586-612.

Academy of Management Journal (2011). http://journals.aomonline.org/amj/information-for-contributors (accessed 10th March 2011)

Alferoff, C. and D. Knights (2009). 'Making and mending your nets: Managing relevance, participation and uncertainty in academic-practitioner knowledge networks', *British Journal of Management*, **20** (1), pp.125-142.

Alvesson, M. and S. Deetz (1996). 'Critical Theory and postmodernism: Approaches to organizational studies'. In S. Clegg and C. Hardy (eds) *Studying Organization: Theory and Method,* pp.185-211. London: Sage.

Alvesson, M. & K. Skōldberg. (2000) *Reflexive methodology: New vistas for qualitative research towards a reflexive methodology.* London: Sage.

Antonacopoulou, E.P. (2010). 'Making the business school more 'critical': Reflexive critique based on phronesis as a foundation for impact', *British Journal of Management,* **21**, pp. s6-s25.

Antonacopoulou, E.P. and H. Tsoukas (2002) 'Time and reflexivity in organization studies: An introduction', *Organization Studies*, **23**, pp. 857-862.

Augier, M. and J.G. March (2007). 'The pursuit of relevance in management education', *California Management Review*, **49** (3), pp.129-146.

Baldridge, D.C., S.W. Floyd and L. Markóczy (2004). 'Are managers from Mars and academicians from Venus? Toward an understanding of the relationship between academic quality and practical relevance', *Strategic Management Journal*, **25**, pp. 1063-1074.

Bamberger, P.A and M.G. Pratt (2010). 'From the Editors: Moving forward by looking back: Reclaiming unconventional research contexts and samples in organizational scholarship', *Academy of Management Journal*, **53** (4), pp. 665-671.

Barley, S.R. and G. Kunda (1992). 'Design and devotion: surges of rational

and normative ideologies of control in management discourse', *Administrative Science Quarterly*, **37**, pp.363-399.

Bartunek, J.M and S.L. Rynes (2010). 'The construction and contributions of "implications for practice": What's in them and what might they offer?', *Academy of Management Learning & Education*, **9** (1), pp.100-117.

Bettis, R.A. (1991). 'Strategic management and the straightjacket: An editorial essay', *Organization Science*, **2** (3), pp. 315-319.

Boje, D.M. (1995). 'Stories of the storytelling organization: a postmodern analysis of Disney as *Tamara*-Land', *Academy of Management Journal*, **38** (4), pp. 997-1035.

Cooper, R. (1989). 'Modernism, postmodernism and organizational analysis 3: the contribution of Jacques Derrida', *Organization Studies*, **10** (4), pp. 479-502.

Critchley, S. (2005). 'Derrida's influence on philosophy … and on my work', *German Law Journal*, **6** (1), pp. 25-29.

Cunliffe, A. L. (2003) 'Reflexive inquiry in organization research: Questions and possibilities'. *Human Relations,* **56**, pp. 983-1003

Cunliffe A. L. (2004) 'On becoming a critically reflexive practitioner'. *Journal of Management Education.* **28**, pp. 407-427.

Currie, G., D. Knights and K. Starkey (2010). 'Introduction: A post-crisis critical reflection on business schools', *British Journal of Management*, **21**, pp. s1-s5.

Das, T.K. (2003). 'Managerial Perceptions and the Essence of the Managerial World: What is an interloper business executive to make of the academic-researcher perceptions of managers?' *British Journal of Management*, **14**, (1), pp.23-42.

Deetz, S. (2000). 'Putting the community into organizational science: exploring the construction of knowledge claims', *Organization Science*, **11,** (6), pp. 732-738.

Derrida, J. (1993). *Aporias.* Stanford: Stanford University Press.

Derrida, J. (1995). '*Honoris Causa*: This is *also* extremely funny'. In E. Weber, E (ed.), *Points … Interviews, 1974-1994*, pp. 399-421. Stanford: Stanford University Press.

Dowd, K. and M. Hutchinson (2010). *Alchemists of loss: How modern finance and government intervention crashed the financial system.* Chichester: Wiley.

Ferlie, E., G. McGiven and A. De Moraes (2010). 'Developing a public interest school of management', *British Journal of Management,* **21**, pp, s60-s70.

Flexner, A. (1939). 'The usefulness of useless knowledge', *Harper's Magazine*, **October**, pp. 544-552.

Ford, J., N. Harding and M. Learmonth (2010). 'Who is it that would make business schools more critical? Critical reflections on critical management studies', *British Journal of Management,* **21**, pp. s71-s81.

Gibbons, M., C. Limoges, H. Nowotny, S. Schwartzman, P. Scott, and M. Trow (1994). *The new production of knowledge: The dynamics of science and research in contemporary societies.* London: Sage.

Godin, B. (1998). 'Writing performative history: The new *new Atlantis*?', *Social Studies of Science*, **28** pp. 465–483.

Grey, C. (2001). 'Re-imagining relevance: a response to Starkey and Maddan', *British Journal of Management*, **12**, pp. s27-s32.

Gulati, R. (2007). 'Tent poles, tribalism, and boundary spanning: The rigor-relevance debate in management research', *Academy of Management Journal*, **50** (4), pp. 775-782.

Hardy, G. H. (1940). *A Mathematician's Apology.* Cambridge: Cambridge University Press.

Jasanoff, S (ed). (2004). *States of Knowledge: The Co-Production of*

Science and Social Order. London: Routledge.

Jones, C. (2004). 'Jacques Derrida'. In S. Linstead (ed.) *Organization Theory and Postmodern Thought*, pp. 34-63. London: Sage.

Kieser, A. and L. Leiner (2009). 'Why the rigor-relevance gap in management research is unbridgeable', *Journal of Management Studies,* **46**, pp. 516–533.

Kilduff, M. (1993). 'Deconstructing *Organizations*', *Academy of Management Review*, **18** (1), pp.13-31.

Kilduff, M. and M. Kelemen (2001). 'The consolations of organization theory', *British Journal of Management*, **12**, pp. s55-s59.

Kilduff, M. and A. Mehra, (1997). 'Postmodernism and organizational research', *Academy of Management Review*, **22** (2), pp. 453-481.

Knights, D. (2008). 'Myopic rhetorics: Reflecting epistemologically and ethically on the demand for relevance in organizational and management research', *Academy of Management Learning and Education*, **7** (4), pp. 537-552.

Lawrence, P. R. (1992). 'The challenge of problem-oriented research', *Journal of Management Inquiry,* **1** (2), pp. 139–142.

Learmonth, M. (2005). 'Doing things with words: the case of 'management' and 'administration'', *Public Administration*, **83** (3), pp. 617-637.

Learmonth, M. (2008). 'Speaking Out: Evidence-based management: A backlash against pluralism in organizational studies?' *Organization* **15** (2), pp. 283-291.

Learmonth, M. and N. Harding (2006). 'Evidence-based management: The very idea', *Public Administration*, **84** (2), pp. 245-266.

Li, D. X. (2000). 'On default correlation: a copula function approach', *Journal of Fixed Income,* **9**, pp. 43-54.

Lipsett, A. (2011). 'Universities braced for heavier research burden' *Guardian on line*, 1 March. http://www.guardian.co.uk/education/2011/mar/01/research-excellence-framework-academics (Accessed 8 March 2011)

Lowrie, A. and H. Willmott (2006). 'Marketing higher education: the promotion of relevance and the relevance of promotion', *Social Epistemology*, **20** (3-4), pp. 221-240.

March, J.G. and R.I. Sutton (1997). 'Organizational performance as a dependent variable', *Organization Science*, **8** (6), pp. 698-706.

March, J. G., and J.S. Reed (2000). 'Citigroup's John Reed and Stanford's James March on management research and practice', *Academy of Management Executive,* **14** (1), pp. 52–64.

Martin, J. (1990). 'Deconstructing organizational taboos: the suppression of gender conflict in organizations', *Organization Science*, **1** (4), pp. 339-359.

McQuillan, M. (2000). 'Introduction: five strategies for deconstruction'. In M. McQuillan (ed.), *Deconstruction: A Reader*, pp. 1-43. Edinburgh: Edinburgh University Press.

Őzbilgin, M. F. (2010). 'Scholarship of consequence: New directions for the *British Journal of Management*', *British Journal of Management*, **21** (1), pp. 1-6.

Peng, M. W. and G.G. Dess (2010). 'In the spirit of scholarship', *Academy of Management Learning and Education*, **9** (2), pp. 282-298.

Rasche, A. (2008). *The paradoxical foundation of strategic management.* Heidelberg: Physica/Springer.

Rasche, A. and M. Behnam (2009). 'As if it were relevant: A systems theoretical perspective on the relation between science and practice', *Journal of Management Inquiry*, **18** (3), pp.243-255.

Reedy, P. and M. Learmonth (2009). 'Are there no alternatives? The contribution to management education of alternative organizations', *Management Learning*, **40** (3), pp. 241-258.

Rynes, S. L. (2007). 'Tackling the 'great divide' between research production and dissemination in human resource management', *Academy of Management Journal,* **50** (5), pp. 985-986.

Shinn, T. (2002). 'The Triple Helix and new production of knowledge: prepackaged thinking on science and technology', *Social Studies of Science*, **32** pp. 599–614.

Starkey, K. and P. Madan (2001). 'Bridging the relevance gap: Aligning stakeholders in the future of management research', *British Journal of Management*. **12**, (s1), pp. s3-s26.

Starkey, K. and N. Tiratsoo (2007). *The Business School and the bottom line.* Cambridge: Cambridge University Press.

Taleb, N.N. (2008). *The Black Swan: The impact of the highly improbable.* Penguin: London.

Tatli, A. (2011). 'On the power and poverty of critical (self) reflection in critical management studies: A comment on Ford, Harding and Learmonth', *British Management Journal*, in press.

Triana, P. (2009). *Lecturing birds on flying: Can mathematical theories destroy the financial markets?* London: Wiley.

Van de Ven A.H. (2007) *Engaged Scholarship: A guide for organizational and social research.* Oxford: Oxford University Press.

Van de Ven A.H. and P.E. Johnson (2006). 'Knowledge for theory and practice', *Academy of Management Review*, **31** (4), pp. 802-821.

Vince, R. (2010). 'Anxiety, politics and critical management education', *British Journal of Management*, **21**, s26-s39.

Weick, K.C. (2001). 'Gapping the relevance bridge: Fashions meet fundamentals in management research', *British Journal of Management*, **12** (s1), pp. s71-s75.

Weitzner, D. (2007). 'Deconstruction revisited: implications of theory over methodology', *Journal of Management Inquiry*, **16** (1), pp. 43-54.

Willmott, H. (2003). 'Commercialising higher education in the U.K.: The state, industry and peer review', *Studies in Higher Education*, **28** (2), pp. 129-141.